deduction and Hamilton's Principle of Least Action. But the limitation is required by the postulate, viz. the Principle of Least Action, not by the deduction. Clausius expressly states (*Phil. Mag.* vol. xliv. (1872) p. 365) that his deduction holds good for systems to which the Principle of Least Action is not directly applicable, and in consequence he claims that his equations involve a new principle which is of more general application than Hamilton's Principle (*vide* Report, § 16). I think there can be little doubt that Clausius had in his mind the very objection which Mr. Burbury now raises, and that it was in order to meet it that he claimed this generalisation.

The assumption as to the conservative nature of the forces is not required except in § 17 of the Report, and at the end of that paragraph two methods of avoiding it are suggested. One is to assume that the force-function can be varied with the time, the other is a method adopted by Von Helmholtz.

If we allow the force-function to be varied with the time, then in Mr. Burbury's case (of a column of gas held down by a piston of constant mass) the potential of *gravity* can be altered and therefore the *weight* of the piston is disposable. This disposes of Mr. Burbury's objection, and it only remains to consider the investigations given at the beginning of § 17 of the Report.

Clearly the Second Law of Thermodynamics cannot be deduced from studying the behaviour of gas under constant pressure. To establish it we must make the working substance undergo a reversible cycle in which heat is absorbed and external work performed. To do this we attach the piston to a crank as in an ordinary steam engine, and make it turn a wheel which raises a weight by means of a windlass. Here we have a strictly conservative system, and one to which the arguments of § 17 are therefore strictly applicable. And for every single turn of the wheel we have

$$\int \frac{dQ}{\bar{T}} = 0 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

a relation identical in form with that which expresses the Second Law.

It seems to me that the real objections to Clausius' deductions are far more intricate and far less easily disposed of. The difficulty of assigning a physical meaning to the quasi-period i is one of them, and there are other difficulties connected with the interpretation of $\bar{T}$ as absolute temperature when intermolecular forces are taken into account. All these difficulties are alluded to in my Report, and they are not peculiar to the hypothesis of "quasi-periodic" motions; similar difficulties exist in some form or other in most so-called "proofs of the Second Law."

It may be interesting to mention that a proof of the Second Law based on the virial equation

$$pv = \tfrac{2}{3}(T + \Sigma\Sigma(\tfrac{1}{2}Rr))$$

was given by R. C. Nichols in the *Philosophical Magazine* for 1876 (v. Series I. p. 369). I hope later on to deal more fully with that portion of Mr. Burbury's letter which relates to the "virial proof."

G. H. BRYAN.

December 21.

Flame.

I HAD hoped that after disavowing the unpleasant interpretation which had been put upon his first letter, Dr. Armstrong would have done me the honour, and himself the justice, of indicating precisely where he disagreed with my scientific arguments. Instead of doing so, he has imputed to me a sensitiveness to criticism so excessive that he feels it best to retire from the controversy with a mere statement that our standpoints are different. I must leave it to the readers of NATURE to judge whether Dr. Armstrong has any longer the right to claim a *standpoint*.

Mr. Newth will find a fuller discussion of my views about flame in the *Journal of the Chemical Society* for 1892, pp. 204-226. If after reading that he still has difficulty in understanding the fundamental points of my work, I shall be glad to help him if he will communicate with me privately. With a little care, Mr. Newth will find it quite easy to separate the two cones of a carbon monoxide-air flame in the ordinary apparatus without the use of a gauze cap. The air must be turned on very gradually. In the case of the hydrogen-air flame it is best to dilute the gases with nitrogen, as recommended in my first paper.

I am sorry that anyone should think I have slighted Dr. Frankland's work. I can, however, understand, and even admire, Mr. Newth's excessive zeal in the matter.

ARTHUR SMITHELLS.

"The Zoological Record."

WE are delighted to find such a consensus of opinion as to the desirability of retaining palæozoology in the *Zoological Record*. The recorders whom we have consulted, the editor, and now the secretary of the Zoological Society, all have expressed themselves in its favour. The question therefore is purely one of finance. Under these circumstances the publication of the correspondence with the Geological Society is of great interest, and the only addition that we could suggest would be the publication in your columns of that poverty-stricken society's balance-sheet.

We should like, however, to point out that the *Zoological Record* appeals to the Geological Society, not merely on the ground of its palæontological contents. Palæontologists go to the *Record* to learn what the neontologists are doing, quite as much as to read the titles of their own papers. Under any circumstances, then, the *Zoological Record* has some claim on the Geological Society, and we must all regret that financial distress prevents the Society from acknowledging that claim.

But the Record Committee of the Zoological Society need not despair; for the *Record* has no less claims on many other of our learned societies, and, by the converse argument, the inclusion of palæozoology merely strengthens those claims. Every biologist should be grateful to those who bring to his notice literature that he would otherwise never hear of. Apart from this, one-third of the volume is devoted to entomology. Why should the Entomological Society not be invited to contribute? Then there are the Royal and the Linnean Societies, and the British Association; the Microscopical, we would mention, did it not already do excellent work of a similar kind. At any rate, surely five of these bodies could be prevailed on to subscribe £20, or even £40 a-piece. The Zoological Society appeals through its *Record* to hundreds of workers who do not belong to it. It has long done an admirable work, of which it will never lose the credit. Everyone should sympathise with it in its present attempt to perfect this work, and should not permit it to suffer so large a pecuniary loss in that attempt.

December 17.

R. I. POCOCK.
F. A. BATHER.

On the Bugonia-Superstition of the Ancients.

LAST August, I published in the *Bulletin Soc. Entomol. Italiana*, 1893, p. 186-217, an article entitled "On the Bugonia of the Ancients, and its relation to *Eristalis tenax*, a two-winged insect." I desire to collect some more materials on that subject, in view of a second edition, and I would be very grateful to readers of NATURE who may be able to give me assistance in that matter.

The information I require may be expressed in two questions:

(1) Whether travellers in out-of-the-way places in Europe or Asia have not come across vestiges of the superstition about oxen-born bees, still lingering among primitive people?

(2) Whether readers of Oriental literature have not come across passages evidently referring to this superstition, like the passage I reproduce here as an example. I found it in the "Golden Meadows" of the Arab traveller Massoudi (died in Cairo, 955), translated by Barbier de Meynard and Pavet de Courteille, Paris, 1861, vol. iii. p. 233. It relates a conversation which took place in Arabia, and of which this is a fragment. "'Had the bees, which produced this honey, deposited it in the body of a large animal,' asked Yiad? The surveyor answered: 'Hearing that there was a hive near the sea-coast, I sent people to gather the honey. They told me that they found at that place a heap of bones, more or less rotten, in the cavity of which bees had deposited the honey that they brought with them.'"

I have sent separate copies of my paper to the Geographical, Linnean, and Entomological Societies in London, to the Natural History Museum, South Kensington, to the Athenæum Club, and to many friends in England. I should be happy to send a copy to anybody interested in the subject.

C. R. OSTEN SACKEN.

The Earliest Mention of the Kangaroo in Literature.

I TAKE advantage of the present opportunity to put another question to zoologists. In the same book of Massoudi, whom I quoted in the previous notice, I found the following passage (vol. i. p. 387):—"El Djahiz, in his 'Book on Animals,' relates that the female rhinoceros is pregnant for seven years,

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during which the cub protrudes the head from the belly of the mother, in order to browse, and withdraws it afterwards. Desirous of being better informed, I asked the people of Siraf and Oman, who visited this country, as well as merchants whom I had met in India. They all told me that the rhinoceros breeds just like the cow and the buffalo; and I do not know where El Djahiz has found this story, whether among his reading, or from his inquiries." This is evidently an obscure tradition about the Australian kangaroo, which had reached some part of Asia, and was connected with the rhinoceros by people who knew nothing about either of the two animals. Has the attention of zoologists been called to this story before?

Heidelberg, Germany, December 5. C. R. Osten Sacken.

On an Undescribed Rudimentary Organ in Human Attire.

Lecturers who are tired of the cockade hat-ribbon and tail buttons, may be glad to know of the following rudiment. The old-fashioned double eye-glass was a folder, with a knob at the outer side of the distal glass; and this on folding locked against a pin on the outer side of the proximal glass. The double eye-glass of the present day does not fold; but, none the less, is the knob outside the distal glass retained for it, though there is no pin to lock with on the proximal glass. How long will it take before this useless rudiment disappears? What will be the cause of its disappearance? As panmixia is out of the question, we may prophesy that it will be economy of material.

Cork, December 12. Marcus Hartog.

EARLY ASTERISMS.[1]

III.

The Constellations referred to in the Myth of Marduk and Tiāmat.

We are indebted to the myth, then, for the knowledge that when it was invented the constellations Bull, Scorpion, Goat, and Fishes had been established.

This argument is strengthened by the following considerations suggested by Jansen:—

"We look in vain among the retinue of Tiāmat for an animal corresponding to the constellations of the zodiac to the east of the vernal equinox. This cannot be accidental. If therefore we contended that the cosmogonic legends of the Babylonians stood in close relationship to the phenomena of sunrise on the one hand and the entrance of the sun into the vernal equinox on the other; that, in fact, the creation legends in general reflect these events, there could not be a more convincing proof of our view than the fact just mentioned. The three monsters of Tiāmat, which *Marduk* overcomes, are located in the 'water-region' of the Heavens, which the Spring Sun *Marduk* 'overcomes' before entering the (ancient) Bull. If, as cannot be doubted, the signs of the zodiac are to be regarded as symbols, and especially if a monster like the goat-fish, whose form it is difficult to recognise in the corresponding constellation, can only be regarded as a symbol, then we may assume without hesitation that at the time when the Scorpion, the Goat-Fish, and the Fish were located as signs of the zodiac in the water-region of the sky, they already played their parts as the animals of Tiāmat in the creation legends. Of course they were not taken out of a complete story and placed in the sky, but conceptions of a more general kind gave the first occasion. It does not follow that all the ancient myths now known to us must have been available, but certainly the root-stock of them, perhaps in the form of unsystematic and unconnected single stories and concepts."

There is still further evidence for the constellation of the Scorpion.

Jensen remarks:—

"A Scorpion-Man plays also another part in the cosmology of the Babylonians. The Scorpion-Man and his wife guard the gate leading to the Māšu mountain(s), and watch the sun at rising and setting. Their upper part reaches to the sky, and their *irtu* (breast?) to the lower regions (Epic of Gistubar 60,9). After Gistubar has traversed the Māšu Mountain, he reaches the sea. This sea lies in the east or south-east. However obscure these conceptions may be, and however they may render a general idea impossible, one thing is clear, that the Scorpion-Men are to be imagined at the boundary between land and sea, upper and lower world, and in such a way that the upper or human portion belongs to the upper region, and the lower, the Scorpion body, to the lower. Hence the Scorpion-Man represents the boundary between light and darkness, between the firm land and the water region of the world. *Marduk*, the god of light and vanquisher of Tiāmat, *i.e.* the ocean, has for a symbol the Bull = Taurus, into which he entered in spring. This leads almost necessarily to the supposition that both the Bull and the Scorpion were located in the Heavens at a time when the sun had its vernal equinox in Taurus and its antumnal equinox in Scorpio, and that in their principal parts or most conspicuous star groups; hence probably in the vicinity of Aldebaran and Antares, or at an epoch when the principal parts of Taurus and Scorpio appeared before the sun at the equinoxes."

If my suggestion be admitted that the Babylonians dealt not with the daily fight but with the yearly fight between light and darkness—that is, the antithesis between day and night was expanded into the antithesis between the summer and winter halves of the year; then it is clear that at the vernal equinox Scorpio setting in the west would be watching the sunrise; at the autumnal equinox rising in the east, it would be watching the sunset; one part would be visible in the sky, one below the horizon in the celestial waters. If this be so all obscurity disappears, and we have merely a very beautiful statement of a fact, from which we learn that the time to which the fact applied was about 3000 B.C., if the sun were then near the Pleiades.

Jensen in the above-quoted passages by implication, and in a subsequent one directly, suggests that not all the zodiacal constellations were established at the same time. The Babylonians apparently began with the easier problem of having six constellations instead of twelve. For instance, we have already found that to complete the present number, between

Scorpio. Capricornus. Pisces.

we must interpolate

Sagittarius. Aquarius.

Aries and Libra seem also to be late additions according to Jensen, who writes:—

"We have already above (p. 90), attempted to explain the striking phenomenon that the Bull and Pegasus, both with half bodies only, ἡμίτομοι, enclose the Ram between them, by the assumption that the latter was interposed later on, when the sun at the time of the vernal equinox stood in the hind parts of the Bull, so that this point was no longer sufficiently marked in the sky. Another matter susceptible of a like explanation may be noted in the region of the sky opposite to the Ram and the Bull. Although we cannot doubt the existence of an eastern balance, still, as already remarked (p. 68), the Greeks have often called it χηλαί 'claws' (of the Scorpion), and according to what has been said above (p. 312), the sign for a constellation in the neighbourhood of our Libra reads in the Arsacid inscription 'claw(s)' of the Scorpion. These facts are very simply explained on the supposition that the Scorpion originally extended into the region of the Balance, and that originally α and β Libræ represented the 'horns'

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[1] Continued rom vol. xlviii. p. 520.

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